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THE REEDUCATION OF AN APHASIC

THE possibility of the speech reeducation of aphasics has long been known and we have several accounts of cases in which after periods of several years patients have practically recovered their normal ability of speech.[1] The method of this reeducation, however, is not known. It is uncertain whether new brain paths are opened in the reacquirement of language and a new habit formed or old paths utilized. There is usually a 'normal' amount of recovery in nearly every case of aphasia, so that a person is able after some days or weeks to speak (if a motor aphasic) or to understand (if the aphasia is of a sensory type) some words or phrases. The suggestion has been made that this so-called recovery is merely the normal functioning of the right side of the cerebrum.

For several months I had the opportunity of observing an aphasic man and of attempting to train him to speak. The patient was a mechanic, aged fifty-seven, who had had a paralytic stroke four weeks before entrance to the hospital on December 12, 1904. His right side had been paralyzed and he was unable to speak. In three weeks after the seizure the paralysis had disappeared but there remained almost complete inability to make himself understood. At first all questions were answered unintelligibly or by 'yes' or 'no.' Later he improved greatly so that sometimes he could answer simple questions regarding himself fairly well, although often his answers were irrelevant and his talk unintelligible.[2] He had no disorder of hearing or of vision. He was often unable to understand spoken or written language, particularly when long sentences were used, and was unable to talk clearly, using many words and naming objects wrongly (*i. e.*, paraphasia). The difficulty in understanding written and printed speech was so marked at first as to

[1] For a recent account of some cases and discussion see C. K. Mills, 'Treatment of Aphasia by Training,' *Journ. Amer. Med. Assoc.*, 1904, XLIII., 1,904–1,949.

[2] It should be noted that the patient was not insane, although he was brought for treatment to a hospital for the insane.

suggest some disturbance in the visual sphere, but there was no hemianopsia, and nothing else to indicate such a disorder. During later examinations the patient was found able to read correctly practically everything, provided he was compelled to pay attention to the individual words, or to the syllables of the longer words. His ability to repeat words, syllables and phrases was very poor. There was evidently a great sensory speech defect, with possibly some motor speech disturbance.[3]

The experiments which were made towards training the patient were comparatively simple, and were chosen not so much to reeducate the man for his daily work as to see how the process of reeducation took place.[4] Attempts were made to get the patient to relearn (*a*) the names of ten familiar colors, of the nine numerical digits and the number ten, (*b*) a short stanza of poetry and (*c*) the Lord's Prayer. Towards the end of the series of experiments I had the patient try to acquire the German equivalents of a few common English words.

Color and Number Education.[5]—A careful examination of the patient soon after his arrival at the hospital showed rather poor ability to name both colors and numbers. There was a tendency to use wrongly the names of colors when describing an object, and often to use the names of colors as names of objects. For example, a piece of glass was called 'green paper.'

In the reeducation of color names ten colored cards, each three inches square, were shown one at a time to the patient. He was asked to name the color of the card placed before him. If this was done correctly he was told 'correct,' if incorrectly, he was told that what he had said was 'wrong' and then he was given a second and sometimes a third opportunity. If after three or four attempts he did not give the right name, he was told the name, and then attempted to repeat it. Sometimes he realized after one trial that he did not know and said so, thereupon the name was given to him and he repeated it (not always successfully, however). The following are the colors which were used: white, gray, black, red,

[3] The patient died May 1 of pulmonary infarcts. There was an old embolus of the left middle cerebral artery which had caused the primary paralysis and the aphasic condition. Examination of the brain showed a softening of the cortex, chiefly of the left first temporal and the angular gyri. A full report of the case may appear later.

[4] At the patient's age it would have been almost impossible to have sufficiently trained him even had he lived for some years. In the cases of reeducation which have come to my notice, all the patients were in the early years of middle life, when new associations are more easily formed than after the age of fifty.

[5] For many of the results in this section I am indebted to Dr. W. F. Roberts, who carried on this part of the work under my direction.

yellow, green, blue, purple, brown and pink. In the daily experiments these cards were thoroughly shuffled so that no special order was used, and the names of the colors could not be associated with the sequence.

The accompanying tables give percentages of correct answers, the number of experiments, the number of mistakes in naming each color and the frequency of the wrong use of names.

TABLE I.

Percentages of correctly named colors.

Date.	Percentage Correct.	Number of Experiments.
Dec. 21–24	43.6	39
Jan. 12–14	45.0	40
Jan. 16–21	72.5	109
Jan. 24–28	84.7	118
Jan. 31–Feb. 4	87.7	114
Feb. 6–10	93.5	107
Feb. 15–18	92.6	108
Feb. 21–23	95.2	63
March 15	95.2	105
March 17	96.1	52

TABLE II.

Incorrect naming of colors.

Colors.	Incorrectly Named.	Name Used Incorrectly.
White.	6	0
Gray.	14	24
Black.	4	1
Red.	12	8
Yellow.	14	6
Green.	4	8
Blue.	8	29
Purple.	6	6
Brown.	18	10
Pink.	18	2
Orange.	—	4
Numerals.	—	5
Miscellaneous (including nonsense words).	—	22

In Table I. the number of experiments, it will be noticed, differed considerably from week to week. An attempt was made to show each color once each day in the earlier part of the work, but when the patient wrongly named a color and was given a second opportunity this has been counted an extra experiment. The gradual increase in ability is marked, and in the three weeks' interval between February 23 and March 15 there had been no loss of the association of the correct word with the colored piece of cardboard. There was a gradual betterment in naming from week to week, with a tendency to approach the 100 per cent. mark. On the last day of this series when fifty colors were shown, in only two cases did the patient make mistakes in naming, and these he instantly corrected.

During the first few weeks the colors most difficult to name were purple, brown and pink. For purple the patient had at first considerable difficulty, not only in naming the color, but also in repeating the name when it was given to him, but once he had acquired the name the color was always thereafter properly named. The other colors showed nothing striking beyond the tendency to mix the words brown and gray. Blue was evidently one of his stock words and was employed under most conditions when there was any doubt or hesitancy.

It was surprising, however, that 'green' and 'brown' were not more often wrongly used. These words were stock words in the early part of the experiments on naming objects. This tendency has been noted before. Very often in naming some objects the patient would say, either immediately or after a short interval, 'brown paper' or 'green paper.'

On February 28 the patient was asked to name all the colors that had been used in the experiments with him. He said: 'brown, gray, brown, yellow, green, red, blue, purple, pink, black, white.' This would indicate that the names of the colors were not entirely associated with the appearance of the cards, and it is probable that there was more or less abstractive ability.

The experiments on number training were made in about the same manner as those on colors. The numbers were about one inch high, placed on cards four inches long and two inches broad. These cards were placed before the patient separately, but in an irregular order, until he had named them, and when mistakes were made there was a procedure similar to that used in the color-naming experiments. In Tables III. and IV. will be found, respectively, the percentages of correctly named numerals throughout the experiments and the misuse of the number names.

The retention of the practice effect for the three weeks' interval between February 23 and March 15 is noticeable as in the case of the color names (Table I.).

TABLE III.

Percentages of correctly named numbers.

Date.	Percentage Correct.	Number of Experiments.
Dec. 20–24	42.3	52
Dec. 26	42.9	14
Jan. 11–14	66.1	56
Jan. 16–21	71.1	152
Jan. 24–28	67.6	139
Jan. 31–Feb. 4	72.8	136
Feb. 6–10	83.9	118
Feb. 15–18	93.5	107
Feb. 21–23	92.5	65
March 15	92.6	108
March 17	92.6	54

TABLE IV.

Incorrect naming of numbers.

Numbers.	Number of Times Incorrectly Named.	Number of Times Names Used Incorrectly.
1	5	2
2	10	8
3	5	15
4	16	5
5	21	14
6	18	28
7	7	40
8	18	33
9	13	39
10	33	10
Miscellaneous.	—	16

At first there was considerable difficulty in naming '10,' the patient often getting the name only after saying '9–10' or '8–10.' A number of experiments were made to determine any number preference the patient may have had, in order to see whether the accuracy with which any number was named depended upon the facility of its

use. The patient was instructed to give at random single numbers, not in forward or backward sequence, until told to stop. Unfortunately he did not understand the condition regarding sequences, for there was a great tendency to give the combinations 3–4–5 and 7–6–5. The number 5 was, accordingly, the one most used. The variations in totals of the other digits were not sufficiently marked to indicate any special preference, and the ease of liberation, therefore, did not greatly differ.

When the figures in Tables I. and III. are compared with the results of experiments upon the acquirement of a new habit, the resemblance is striking. In forming new associations or habits at first the progress is slow, and the habit is perfected gradually. The curve of efficiency rises by little and little until it reaches its maximum point, *i. e.*, to one hundred per cent. of right cases in experiments similar to these. In the reacquirement of an old habit, the course runs somewhat differently. Consider the case of one who has not had an opportunity to swim or skate for some years. The first time after this period of rest he goes into the water or attempts to strike out on the ice the adjustment of the muscles and the movements necessary to properly perform the acts are not very accurate, but with very slight practice most of his former activity returns. In the case of this patient we find that this process, *i. e.*, sudden return to former efficiency after little practice, has not gone on, but that the gradual betterment is more like the acquirement of a new habit. This would indicate, it seems to me, that not all the old brain paths were being reopened, but that in all probability new connections were being made. In harmony with this view we have the fact of the 'vicarious' function of the cerebrum, that one part or one hemisphere may take up the lost functions of another part or side. It seems probable that this is what happens always in cases of aphasia, in which there is any amount of reeducation.

That new brain paths may be broken through is evident from the few experiments made to get the patient to acquire the German equivalents for the words 'pencil' and 'pen.' The patient, it is said, had no previous knowledge of German. The word for pencil (*Bleistift*) was shown to him, the pencil put before him, and he read the word. After each five readings the word was covered up, the pencil was removed and he was asked to give the German word for 'pencil.' In reading the word he made many mistakes at first, for example, *Bleistiff, Bleiskiff, Bleistaft,* and a combination of *Blein* or *Blant* with one or other misreadings of the second part of the word. In the recall of the word similar errors are noticed. In the same manner experiments were made with *Feder;* the word was shown and a

pen placed on the table in front of the patient. After five readings of the word it was covered up and he was asked to tell what it was. The results of these tests are as follows:

Day 1, A.M. Pencil: Experiments[6] 1, 2, 3—*Bleinstift;* 4—*Blantstift;* 5, 6, 7—correct. Twenty seconds later he gave the word as *Bleiskiff.* Pen: Experiments 1, 2—correct. Thirty seconds later he confused this word with the one for pencil and said *Blanskiff.* Five hours later when asked what pencil and pen were, he had forgotten, although he had retained the word *Feder* for an hour. When the word *Bleistift* was shown to him he remembered that it was the equivalent for pencil.

Day 1, P.M. Pencil: Experiments 1, 2, 3, 4, 5, 6, 7—correct. Fifteen seconds later, *Bleisplift;* after forty seconds, *Bleiswift;* after forty minutes, *Beltfed;* and after an hour, *Befat.*

Day 2, A.M. When asked the equivalent of pencil, he said *'Fils,'* and pen, *'Fre, Fra, Frain.'* When the word *Feder* was shown to him he immediately said 'pen.' On this morning twenty-five readings of each word were made by him, and after each five there was a correct reproduction. Twenty minutes later he called a pencil *'Bleiswift,'* and twenty-five minutes after the final repetitions of *Feder,* he said 'pen is *Swe-id.*'[7] Fifteen minutes later he recognized the word *Bleistift* and gave it its proper meaning.

Day 2, P.M. Before any experiments were made in the afternoon the patient was asked 'Pen'? and replied *'Be';* 'Pencil'? and replied *'Methfas.'* When shown the word *Feder,* he again immediately said 'Pen.' At this sitting two experiments were made with pen, and three with pencil; all the results were correct. Twenty minutes later pen = *Feder* and pencil = *Bleiswift,* and the same answers were given an hour after the repetitions.

Unfortunately at this point the experiments were discontinued before the words had been perfectly learned. The results that were obtained indicate, however, the possibility of the formation of new association speech paths, and the rapidity of assimilation of the two words, show the feasibility and quickness of the reeducation process.

The Acquirement of More Complex Speech.—Many neurologists recommend the training in sounds of vowels and consonants, and the training in the different parts of speech before more complex material

[6] Each experiment, it will be remembered, consisted of five readings and the immediate reproduction, as soon as the printed word and object could be taken away. The reproduction is the result noted in each experiment.

[7] It should be remarked that many of these nonsense words can not be accurately represented here. It was extremely difficult to transliterate such unfamiliar sounds, but I have given what appeared to me to be the nearest approach to what the patient said.

is tried. To see how difficult or easy it would be to acquire long sentences and many ideas at once, an attempt was made to have the patient memorize the first stanza of the well-known poem, 'The Village Blacksmith.' The conduct of the experiments was as follows: a part of the stanza, one or more lines, typewritten, was put before the patient. This he read aloud five times, and on its withdrawal he attempted to repeat it. This went on, five readings and then a memory test, until he had several successive times correctly reproduced the part. Then another part was placed before him, with alternate five readings and memory test, etc. A tabular account of the results follows:

First day. Whole Stanza, 5 experiments (*i. e.*, 25 readings and memory tests); result of fifth experiment: "Under the sinews chestnut tree are brawn beside the tree—brawny man with sinewy hands —brawn hands and muscles of broadened hands."

Second day. Line 1, 6 experiments: 2, 5 and 6 correct; line 2, 6 experiments: 2, 5 and 6 correct; lines 1 and 2, 12 experiments: 9 correct.

Third day. Lines 1 + 2, 8 experiments; and alternately lines 1 + 2 and line 2, 9 experiments each: first line given well throughout the series of the combination test; second line given well alone, but not in combination except in experiments 22, 26 and 27. Line 3, 4 experiments: all correct. Line 4, 4 experiments: all correct. Lines 3 + 4 alternating with line 4, 5 experiments each: line 4 remembered poorly alone as well as in combination; experiment 11 (lines 3 + 4), memory correct.

Third day, P.M. Memory of the four lines before experiments were begun: "Under the chestnut tree—springing—no—under the springing chestening tree the village man—man is a mighty man was he." Lines 1 + 2, 6 experiments: 3, 4 and 6 correct reproduction; lines 3 + 4, 7 experiments: 5, 6 and 7 correct memory. Lines 1 — 4, 10 experiments: the best reproduction (ninth experiment) was as follows: "Under the spreading chestnut tree, the village smithy stands. The man, a mighty man is he, with powerful hands and—."

Fourth day, A.M. Lines 1 — 4, 5 experiments, and alternately lines 1 — 4 and lines 3 + 4, 10 experiments each: the best results for the four lines were after experiments 2 and 4. In these two tests there was a mistake in only the third line as follows: "A man—a mighty man is he."

Fourth day, P.M. Lines 1 — 4 and lines 3 + 4 alternately (8 experiments each and one extra 1 — 4): after the ninth experiment, result similar to the morning trials, the other tests were not so good. Each time lines 3 + 4 were used alone, however, the memory for these was perfect.

Fifth day, P.M. The memory for the four lines before any reading experiments were made was as follows:

A. "Under the spreading swing tree
The villing wishy man.
A man—a mighty man is he
With long—and vigoury."
B. "Under the side of the spreading tree
The wising—the village smithy smelt
The man, a mighty man is he
With—and many name."

Only two experiments were made on this day, lines 1 — 4, the result of the second being like that of the best on the previous day.

No further attempt was made to proceed to lines 5 and 6, as it seemed probable that in the combination of the last two lines with the four previous ones there would be an exaggeration of the difficulty in remembering. The results obtained, however, indicate that the learning process in this patient took on the same character as does the learning by normal subjects. The greatest difference is in the number of repetitions necessary to break through smooth brain paths. The results indicate, in addition, that reeducation is not such a difficult matter, and that a few days' training may be sufficient to change a patient from a condition of very feeble speech ability to one in which he could express his ideas well enough to have his wants attended to. Moreover, this would be accomplished more easily had we information regarding such a patient's habits, likes and dislikes, previous education, books read, etc.

That this last statement is true is borne out by the results of training the patient to rememorize the Lord's Prayer. I desired to work with something the man had formerly known well, in order to ascertain whether or not it took so long as the previous work. Before he read the prayer his memory reproduction was: "Our heavenly Father, in thy name—thy kingdom come." Three readings on the first day with memory test following each did not seem to greatly improve this rendering, but on the following day after three experiments (two readings each) the patient left out very little ('who art in heaven') and became confused in part ('give us each day our daily bread and to do with . . . do our doubts with our debtors without our duties'). The same afternoon two experiments were made, each after two readings, with the result of having the same part confused. On the following morning before reading the patient left out 'lead us not into temptation,' but did not greatly confuse the rest of the prayer. Twelve readings were then made and the memory tested after each two readings. The final result after these experi-

ments was a slight mistake, 'lead us under temptation.' About five hours later after four readings the whole prayer was reproduced accurately and without hesitation.

In number of words this prayer is three times as long and in number of ideas twice as complicated as the four lines of 'The Village Blacksmith,' but the subconscious remnants of the prayer, coupled perhaps with a greater interest, served to make the patient master of this composition after twenty-five readings.

The results of all the experiments indicate the relative ease of reeducation in conditions of aphasia, and it appears advisable to attempt such reeducation whenever the physical condition of the patient permits. There is danger of too great mental as well as physical work in some aphasic cases and a too rigid course of training should not be attempted. Aphasias due to emboli could, of course, be more actively treated than the cases due to cerebral hemorrhage. In all cases in which there is a possibility of training, a few minutes each day, morning and afternoon, would soon produce noticeable betterment. The experiments which have been carried on with this patient seem to indicate that new brain paths are opened in the reeducation process, and in this work it is probable that the right side of the cerebrum takes part. More experiments are needed (and on more cases) along the lines of the formation of definite speech associations before the latter conclusion can be conclusively drawn.

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DISCUSSION

IMMEDIATE EMPIRICISM

PROFESSOR BAKEWELL writes as follows, in an open letter to me in this JOURNAL concerning 'Immediate Empiricism':[1] "My difficulty, in short, is simply this: Either everything experienced is real exactly as, and no further than, it is then and there experienced—and then there is no occasion to speak of correcting or rectifying experience; or, there is in every experience a self-transcendency which points beyond that thing *as experienced* for *its own* reality—and then good-by to immediatism." And in a foot-note he says that my view is atomistic, chopping reals off from one another, and that if 'this consequence is avoided by making the earlier experience con-

[1] Vol. II., No. 19, p. 521. See also this JOURNAL, Vol. II., No. 15, p. 393.

tain implicitly the later to which it leads, immediatism gives way to a doctrine of mediation.'

There was once a botanist who suggested that instead of deducing botany from the concept of plants (and from certain allied concepts) the proper method was to study plants to see what each was in itself. Whereupon an opponent replied that such a doctrine destroyed botany. "Take the case of a seed"; he urged, "either you mean that this seed just as it now is, and no further, is real, and then growth is impossible; or else there is in the seed a self-transforming somewhat which changes it first into a sprouting plant, and then, finally, into a mature plant with seed of its own—and then good-by to the idea that the reality of the seed is to be sought in just what the seed now, and no further, is. Moreover," he continued, "since each plant in itself is something different from every other, the doctrine makes relation of plants to one another, and hence generalization, and hence science, impossible."

Whereupon the first-mentioned botanist replied that either a given seed is alive and capable of growth, or, dead and incapable of becoming a plant, and that the actual state of affairs in this respect is precisely one of the things to be determined by a study of the particular seed; that it is of the very essence of the method that the question of 'further' or 'no further' should be settled by reference not to general notions, but by reference to the *determinate* character of the particular seed. Moreover, it was just by a study of each plant 'in itself' that one would find out whether it was something unrelated, atomistic, or something genetically and responsibly connected with other plants, relationship being precisely an affair of the determinate character of the seed.

In other words, while I expressly state in my article (1) that a thing which is rectified in a subsequent cognitive experience 'contains within itself' (that is as part of its own concrete determinate thinghood) 'the elements of the transformation of its own content,' and (2) expressly disclaim the possibility of deriving any conclusions whatever from the concept of immediate experience, Professor Bakewell expressly assumes (1) that the very *concept* of immediate experience carries with it some necessary implication regarding the character or nature of *what* is experienced, and (2) that it precludes any continuity of experienced things. As an immediate empiricist, I can only reply that it is to things *as* experienced that I go for instruction as to continuity, transformation and mediation; and that it is just because I find things immediately experienced *as* continuous, and *as* self-rectifying that I believe in continuity and self-rectification. Compare the distinction of cognitive and cognized

in the former article, and the reference to the importance of the 'drift, occasion and contexture' of things—distinctions which are inherent and not external to the things. Does the transcendentalist believe that things *as* experienced are continuous? If yes, why should he charge an empiricist with *ex officio* denial of this empirical fact? But if he holds that a transcendental principle or function is required to give continuity to what as experienced is 'chopped-off,' then *he* would seem to be the one denying actual, empirical, continuity. I am always wishing that some transcendentalist would expound and expose his own positive doctrine about the problems which he accuses the empiricist of maltreating, instead of assuming that the transcendental position is self-evident, or at least thoroughly understood. Perhaps Professor Bakewell will help in this illumination, bearing in mind that an important motive in developing the newer philosophy has been the conviction that mediation, continuity, reconstruction and growth are facts which transcendentalism has failed consistently to define and account for. I do not understand the notion that because things of immediate experience are real, mediation can not be real. I am quite sure that the logic of immediate empiricism would include mediation along with the categories 'subjective, objective, physical, mental, psychic, etc.' (see Vol. II., p. 399) and say, 'if you wish to find out what it means, go to experience and see what it is experienced *as*.' I find difficulty in realizing the difficulty which one has with immediately experiencing something *as* mediate. I don't see *any* way of experiencing the mediate (any more than of experiencing a cat or a dog) excepting that of immediately experiencing it *as* what it is, viz., mediate. If I were to make a guess as to the origin of the difficulty, I should refer it to a mental habit of employing a conceptual, instead of an empirical, philosophy,[2] a habit so inveterate as to display itself even when one is attempting to appreciate the position of an empiricist.

I conclude with a question and a remark: Does Professor Bakewell mean to deny (1) that all philosophic conceptions must somehow enter into experience, or (2) that all experience is, *as existence*, immediate? The remark is, that I quite meant my earlier statement, (Vol. II., p. 399), that from the postulate I gave, not a single philosophical proposition could be deduced—that its significance was that of affording a method of philosophical analysis.

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[2] Lest I be charged with intimating that concepts are unreal and unempirical, I say forthwith that I believe meanings may be and are immediately experienced *as* conceptual.

UNIVERSALS: A CRITICISM

THE study of concepts is no longer in its infancy, but it does show now and then headlong tendencies to extremes, which is perhaps indicative of a lusty youthfulness freshly entered upon. The most absorbing fad—pardon the word!—in present-day logic seems to be the ultra-psychological one. I do not say ultra-empirical, much as many of those who pursue it believe that it really is this. But empiricism of to-day is thoroughly antidualistic, and this can not be said of all the 'logical' theories of concepts. I have in mind an especially interesting paper on universals by Dr. Sheldon which strikes me as illustrating most excellently the ultra-psychological tendency which carries us away from every sort of empiricism that I know of. While agreeing verbally with the writer on many fundamental points, such as that of the 'concreteness' of universals, for instance, I find upon analysis that the harmony does not get beyond the rhyming stage; for what he means by his terms are not things which I can find to be an empirically justifiable interpretation. Just because the differences between us are so heaven-wide, I have ventured to formulate them here in the firm belief that whatever mistakes either or both of us have made will come to light by some such systematic comparison. And, inasmuch as I can agree to many of the statements Dr. Sheldon makes, and am, for all that, unable to fit them together as he does, it is pretty certain that there are verbal equivocations which are 'playing horse' either with him or with myself. The question then is, What are they, where do they lurk, what havoc have they wrought?

After mentioning the views of Bradley, G. E. Moore, and Royce, Dr. Sheldon states that they rest upon a misconception, and expresses his own conviction that the universal 'is rather a definite presented aspect of concrete experience, something we can observe and identify.'[1] And further on: 'All the properties of a universal, all that it actually means to us, is worth to us, can be defined in concrete terms'; it is, moreover, 'just as fully concrete as the individual event in time, and as such has as good a metaphysical status as the latter.' To all of this I can subscribe to the letter, but I fear my letters spell different words from those the writer makes them combine into. The difference in interpretation hinges here upon what we are going to call a presented aspect of concrete experience. To my mind, concrete experiences are experiences of concrete things, which latter are presented in various aspects. The processes we find upon reflection and experimentation to mediate knowledge of such things are them-

[1] *Philos. Rev.*, XIV., 196.

selves concrete experiences or aspects of such only when we attend to them, discover them by comparison and analogy. In this respect, such processes stand in virtually the same relation to knowledge as the psychophysical ones (in the narrower sense of neural transformations) do. Taking this standpoint, it becomes impossible to regard anything as a present aspect of an experience unless it is something that is presented in and through the experience. What we find retrospectively to have been an element in the process of experiencing something is now, in our new reflective experience, a presented aspect of the object in mind, viz., the process of the past mediation. In other words, I call an aspect of experience only that which we can observe and identify in what we experience. The '*mouches volantes*' which I can note while looking at the house across the way are not aspects of the house, but merely other things given along with the house in their own peculiar way; mere coexistence in the same experience-moment does not lead me to relate them, but does force me to treat them as aspects of the whole experience-content. Put briefly, then, concrete experience is not the detailed analysis that scientists, viewing it from a special, peculiar standpoint, may render; it is nothing save the intended things *as intended.* We have full right to inquire into this intending process, provided we do not perpetrate the vast blunder of supposing that our analysis of it somehow changes the nature of what is actually intended. What is changed as a result of our new studies is solely the value of the intended things; for knowledge of the limitations of how they became intended enlightens us as to their use in the next case. For instance, we may have really meant something which does not exist in the way we held it to, and a thorough insight into the nature of thinking processes may warn us away from intending non-existent things. A case in point is the universal, in the sense Dr. Sheldon means it. To it we now turn.

The method of approaching the universal adopted by Dr. Sheldon impresses me as extremely hazardous. He invites us to "consider the term 'red apple.' What are the facts when we entertain this, and recognize that it is a general term applying to many possible instances?"[2] As I look at the matter, the case we are called upon to consider here is not the correct one, chiefly because the recognition of the words as a general term applying to many possible instances is neither an essential phase of the process of thinking a universal nor yet a peculiarity of the universal itself. In other words, the situation above portrayed is not one in which a universal is given in its normal sense at all, but is viewed from the standpoint of the individuals falling under it, which individuals in turn are not viewed as

[2] *Ib.*

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such, but rather from the psychological standpoint of so many mental events. Let us see whether this criticism can be maintained.

There will be no discussion over the fact that the origin of the idea of a given universal, *i. e.*, the way the universal comes to be known, is something wholly different from the 'content' or thing-character of the universal itself; and further that an equally great irrelevance obtains between the way the universal is first built up in knowledge and the way it recurs as a familiar thing in habitual thinking. The one thing American pragmatists have put beyond all intelligent controversy is just this double fact. Accept it, though, and you can not go ahead with a study of the way a universal is experienced until you have prescribed which type of experience you intend to analyze. Every known thing being mediated, it appears that there are three mediating processes, one for the *rise* of the thing in knowledge, one for its *adequate definition,* and one for its *functional use* in 'situations.' This latter process is plainly a generalization, inasmuch as each distinct use of a thing probably entails a corresponding variation of process. Another highly significant difference is to be found between the process of understanding a universal directly communicated and that of thinking a universal in one's own thought, *i. e.*, by association, inward suggestion or the like. And there may, for all I know, be other even more important types of mediation-processes.

Let us suppose, now, that we want to study the status of the universal. Plainly, if we are going to take the processes of mediating it into account in the belief that these somehow affect the standing of the mediated thing, we put ourselves under obligation to investigate all process-types alike. For we have no reason *a priori* for supposing that any one process is *'realler'* or more significant than any other one. Therefore we would have to ask ourselves: What are the facts when a series of experiences culminates in bringing to knowledge 'red apple' as a wholly new thing? What are they when we attend to this new thing and define it? What when 'red apple' functions in connection with other things to bring forth new knowledge? What when it merely appears as a suggestion, in a 'fringe,' etc.? And an indefinite number of other similar questions, the number of which no man knoweth. All being finally answered, some sort of comparison or reciprocal evaluation would have to be made—but from what standpoint, if not from that of the particular universal *as meant,* I do not know. Now, Dr. Sheldon has discussed only one very special process, namely that of mediating the *definition* of a universal. In ordinary life we rarely recognize the applicability of the thing in mind *as a term* to many possible instances; such a recognition occurs perhaps in the thinking of logicians and lexicographers now and

then, but not in the intellectual operations of ordinary people with the things meant. Indeed, the applicability of a term to many possible instances has, for the common man, absolutely nothing to do with the fact that many universal things have numerous individuals 'under them.' Looking at the facts as best I can, I would say that the 'universality' of 'red apple,' far from having anything to do with the ways I can use these two words to label various 'experiences,' is merely my way of indicating with a positive predicate the fact that *a red apple, so far as I know and mean the thing, contains no characteristic or peculiar function that can regulate the appearance of the fruit in consciousness.* And when I think of 'red apple' as 'containing under it an indefinite number of individual red apples,' I mean merely that, *when I think of a red apple without further qualification (than redness), I have placed no restrictions upon individuality, i. e., have not so prescribed the functions of the apple in question that only one particular apple could function to illustrate what I mean.* I never refer *positively* to other concrete instances, other individuals, other processes, or the like; the very fact that I refrain from so doing contributes to make the meaning of 'universal quality' as I think of it reflectively. But, entirely apart from what I believe here, would it be a good explanation of the concreteness of the universal if I were to study my mental processes in presenting the above 'definitions'? I fear not. And yet just such a procedure is found in Dr. Sheldon's approach to the problem, as a further study of his analysis will disclose.

Accepting the particular experience-type offered us, we find first of all, it is said, a concrete image, to which is added 'a recognition that this motor response, this image, this phase of the present process, will apply as well to various other possible situations as to the present one; this is what we signify when we speak of the intent to refer to other similar possible experiences besides the present.' But let me ask anybody who has not been convinced that mere 'mental states' can explain everything, whether he really does recognize anything like this whenever he experiences a 'universal' thing. Surely, not even the most highly trained psychologist ever reaches this point. But what is true is that if we look at a universal as a general term applying to many possible instances, in short if we look at things called universals from the lexicographical or grammatical standpoint, we may well recognize that what we now have concretely 'in mind' as the definition of the terms in question will do quite as well to mediate the thing to us at any moment when our mental organism is substantially what it now is. Thus, the recognition of applicability to other instances is also the psychophysicist's recognition of the inadequacy

of the present mediating image, motor response, or the like to exhaust the meaning of 'universality.' For the words 'red apple' are, when experienced sensationally or perceptually or imaginatively, not the things meant, but solely tools to thought, symbols of genuine significances, so that what goes on in understanding the significance of words is by no means the experiencing of the things we critically call universals. It is easy to see in all this where the root of the age-long controversy between realism and nominalism (in the scholastic sense) lies; and the modern disputes about concepts draw their nourishment from the same evil stem. The difficulty is not so much how many things can be classed together in the basis of common characteristics as it is how, in meaning the class-basis as such, I can do so without simultaneously having 'in mind' somehow or other all the possible things from which the class-characteristic is drawn. And Dr. Sheldon admits just this to be the cause of the trouble when he discusses the nature of 'possibility.'

He explains the 'possibility of other instances' as necessarily a concrete aspect of the universal-experience; 'we feel that it must refer to something real, some real character about our experience.'[3] And, driven on by this conviction, he succeeds in finding that, in the case of seeing a red apple, there is a suggestion of other red apples or images of them; "the faint motor response of grasping and eating suggests that a similar response may occur again. The suggestiveness, however, is neither more nor less than what we mean by the phrase 'the possibility of further similar cases.' " All of which is to me a desperate attempt to maintain a theory and nothing more, for I can not find in any of my faint motor responses to red apples or other things any suggestion that there may be more responses like them later on. Still less is this what I have ever meant when talking about the possibility of further similar cases; were I to speak of such a possibility in connection with red apples, I would mean merely that, so far as I know anything about red apples and their laws of growth, geographical distribution, market demand and so on, there is no tangible positive reason adducible why there may not be apples of that sort to-morrow, next year or on some other star countless æons hence, nor why there may not now be millions of apples which absolutely no psychic organism comes into any true connection with. Perhaps in the very act of thinking the above sentence there were many images haloed with pointing suggestions; but if there were, it was not this tendency toward the fringes that I meant by 'possibility.' And let me here ask the reader whether he can identify thinking-a-thing-possible with the experiencing of things (images,

[3] *Ib.*, 197.

responses, motions, etc.) in a way leading up to the 'notion' of possibility? Personally, I am unable to.

We are told explicitly, however, that 'all that the universal means to us, all that is actually of any use in our thinking, lies in the fringe of suggestiveness that accompanies one particular case.'[4] This statement contains in a condensed form all the most serious equivocations in the entire problem; to detail them becomes highly necessary to every final mutual understanding. First, the universal may be construed either as the particular thing referred to in thought and reflectively called a universal, or as the mere words symbolizing any member of a class, or finally as the peculiar indefinite quality of the thing which makes it non-individual and 'abstract.' In the first sense, red apples do not lie in any psychic fringe, but usually hang on trees or stand in barrels down cellar; in the second sense, the words unquestionably do consist largely of suggestive elements of various kinds which aid and supplement the merely sensed words in conveying their socially fixed meaning; and, in the third sense, the 'abstract' character of 'red apple' is a mark of the mere lack of those qualities and functions which, from whatever standpoint we take, force us to think of some one object in space or some one in time (*e. g.*, a mere momentary 'experience'). Inasmuch as my own training, or lack of it, makes me speak of a universal in the first sense mentioned, I can not agree with Dr. Sheldon's remarks about where it lies. The second equivocation lies in the meaning of 'use in our thinking'; the things that are of use to *us* are, of course, various objects we have come to know, but we do not use them in our thinking in quite the same way that we use a pen in writing. The things themselves are of use in *doing* something; but thinking itself is, properly speaking, only a tool for doing something too.[5] But it differs from other things in that it is the medium through which all these latter become significant, effective, and valuable to us. Hence there is a perfectly good sense in which we may say that all that red apples mean to us lies in the fringes of suggestiveness accompanying the acts of thinking them; that is to say that we reach things as 'complexes,' 'centers of activity' or the like through other media than mere sensations. And universal things are like all others in this respect. But it is a very different statement that makes the 'quality' of universality consist in a certain sort of feeling, full, pointing or the like. If a universal meant only this, then as soon as we discovered the fact all utility would vanish. The odd thing is that psychological logicians often fail to see that what lies in suggestive fringes is not merely a bundle

[4] *Ib.*, 202.

[5] 'Doing' here means every sort of *directed* activity.

of tendency-feelings, but quite as often perfectly definite things which in themselves are no more mere mental states than red apples are. This failure, of course, traces back to the venerable error of an antiquated sensationalism which held that, inasmuch as the only true and real things we know are reds and hots and moves and pains, hence things can be really 'given' only in sensational form. Unfortunately, there is no space to discuss the classical perversion here. It underlies, however, the third great equivocation in the statement we are now discussing; the 'fringe' may mean either 'the other things or the other characteristics of the same thing in mind' or else 'the processes mediating these other things.' And the suggestiveness of the fringe is, in the former case, the objective relation between the sensationally given thing and the things otherwise given, and in the latter a feeling of tendency or pointing which somehow means something more than direction, means a *terminus ad quem* in the shape of other instances. For my part, though, I am unable to find in simple feelings of tendency, pointing or movement any implication of a *terminus ad quem;* such implications are merely habitual additions growing out of reflection upon past movements that actually did reach a definite end. The mere process-feelings, then, are not the meanings the logician is studying.

What convinces me most strongly, however, that this ultra-psychological theory is not discussing the logical problems at all, is the fact that Dr. Sheldon's aim is 'to define the universal in relational terms; as something which has a tendency to lead us on to further cases.'[6] Such a program, however much it can be justified as a psychological one, does not give even a campaign promise of 'better things' in logical inquiry. For it does not distinguish universal things from other things, inasmuch as all things have precisely this same general suggestiveness, so far as I see. Everything is apt to 'recall' some other, and this process is not backward, but forward. The recall is never a mere atavism of mind. But what distinguishes the universal from other suggestive things? Surely nothing in the suggestion-process, at least so far as psychology thus far has disclosed, but solely in the thing itself, the 'red apple,' 'man,' 'potatoes,' 'infancy,' etc.

In conclusion, note the following theoretical dualism underlying the extreme psychological interpretation. The meaning 'red apple' is first grasped, understood in the common-sense way, and analyzed *as a thing,* whereby it is correctly found that no definite red apple nor group of such is meant, that no particular shape, size, flavor, or shade of red is specified, and that the present experience, *as a process of mediating,* does not limit the meaning at all, but leaves open the pos-

[6] *Ib.*, 200–201.

sibility for other processes to mediate the same identical significance (thing). Now, after having thus gained the meaning of 'red apple,' the psychologist seeks to make this meaning 'concrete' by showing that there are phases in the process of experiencing 'red apple' which correspond to the phases in the meaningful thing 'red apple.' But what is this attempt if not one to set up a quasi-empirical 'one-to-one correspondence'? Instead of talking about the parallelism between mental and neural states, the psychological logician offers us a parallelism between the phases, qualities, and functions of things and the moments in the noetic psychoses that bring these things to knowledge. I see no less absurdity in this than in an assumption that an experience-process of mediating a geometrical figure has as many organic movements as the figure has sides, angles and areas. And in the last analysis such a theory plays the traitor with just those data of pure experience which it professes to make use of, namely the immediate, simple facts of movement, succession, transition and other things which critical analysis shows us to be highly complex *objects*. A taboo is placed upon the now generally accepted view that a complex meaning may appear in an irreducible experience, and *vice versa*.

The claim has often been made that the many discrepancies between what we think and what we experience in this thinking are to be accounted for by the fact that active thinking processes skip along above the speed-limit of introspective description. And, in the present case, it might be urged that the recognition of the response-function and the tendency-feeling is unconsciously present or absorbed in the thought of the 'thing in mind.' Yet is this not a makeshift? It is true that thinking processes are, when compared with all the numberless sensations, feelings, reactions, etc., underlying them in one way or another, 'short-cuts'; but they are such simply because these underlying 'mental states' do not belong to the things thought about to whose rise in consciousness they have nevertheless contributed. In explaining the status of universals, therefore, it profits us to pay little heed to their transmission, analysis of which has its own uses, but no logical ones,—and still less to metaphysical implications about the transmitted things.

After so much destructive criticism one has a right to ask what can be offered as a positive theory of universals to supplant the ultra-psychological interpretations. Such a theory must be developed elsewhere, but it may be faintly forecast here with the remark that a careful study of what we really mean by a 'thing' reveals, on the one hand, a 'center of function' (somewhat as Lord Kelvin described the molecule); and as a result of such an interpretation we find that what we call individual objects in space are primarily the true 'ab-

stract universals,' being the 'effects' of countless simpler objects which a misleading terminology has labeled 'mere sensations,' 'feelings' and the like. If this forecast rings riddlesome here it is probably because the fallacies of conventional psychological language have not been appreciated. But that it is the consistent implication of modern empiricism, I have little doubt.

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REVIEWS AND ABSTRACTS OF LITERATURE

Logic, Deductive and Inductive. JOHN GRIER HIBBEN, Ph.D., Stuart Professor of Logic in Princeton University. New York: Charles Scribner's Sons. 1905.

The task of preparing a modern text-book in logic lays under large contribution the resources of the historical and scientific as well as of the philosophic spirit. To rehabilitate what remains of the traditional formal logic that is worth preserving and handing down to another generation, to search out and to systematize essential methods or tools of scientific procedure, often employed as they are almost unconsciously and in fields remote and technical, and finally, to bring home to the student a fruitful conception of the philosophical significance of logic, a realization of its catholic scope and abiding interest as an inquiry into the dialectic, the drama of thought itself—these are concerns which the maker of a text-book on logic may be expected to reckon with. This much, at least, must be said in appreciation of the many-sided and comprehensive treatment which logic has received at the hands of Professor Hibben in his book on 'Logic, Deductive and Inductive.'

It would be difficult to think of a logical topic of any importance that does not receive attention in these pages. The reader renews acquaintance with the ancient forms of traditional logic and observes with interest how greatly some of them have been rejuvenated by the garb of modern instances and unworn illustrations. The author is to be congratulated on the success with which the 'stock examples which have grown old and infirm in the service of many generations of students in logic,' to quote from the preface, have been eliminated, and their places supplied by illustrations and examples 'taken from the sphere of every-day experiences, in order that they may represent modes of actual reasoning pursued by the common run of mankind.' It has been the aim of the author, we also read in the preface, 'to present in connection with the more formal and traditional treatment of the deductive logic also some considerations which have been contributed by the discussions of the modern logic and which find expression in such works as those of Sigwart, Lotze, Erdmann, Green, Bosanquet, Venn and others.'

The book, as a whole, consists of two parts, the Deductive and the Inductive Logic. The latter part is a revision of the author's 'Inductive Logic' which was published in 1896.

Part I., the 'Deductive Logic,' opens with a series of chapters on the nature of thought, the concept, and the judgment. The wisdom of dwelling in advance and at some length on the nature of thought is easily apparent, although the definition of thought laid down at the outset be confessedly incomplete, if not unduly one-sided and abstract. "Thinking . . . may be defined in one of its aspects at least as the process of interpreting the special by the general, or the new experience by the old" (p. 4). True, inasmuch as 'thinking' is to be the subject-matter of the whole book, it may well be let off with a very incomplete account of itself in the first chapter. But stripped of its antecedents and occasions, broken off short on its stem, it presents, in spite of a suggestive illustration which I have not the space to quote, a rather limp and devitalized appearance, as if chilled by the rigor of the severely logical atmosphere into which it has been suddenly thrust. It is considerably thawed out and revived, however, through well-chosen illustrations in the immediately following discussion of the four functions involved in reflection: (1) The formation of concepts out of the 'crude data of knowledge furnished by the senses.' (2) 'The reduction of the total mass of concepts to some kind of systematic order.' (3) Interpretation—the referring of the object of thought to its appropriate concept. (4) Inference—the process of unfolding what is implied in judgments.

In these introductory chapters judgment, of course, is given the central place. It is made clear that the concept can lead only an empty and precarious existence unless employed in the act of judging, in the act of qualifying, or interpreting, some object of thought, whether perceptual or conceptual. Considerable space is devoted to the drawing of distinctions between the empirical and the logical concept, emphasizing the growing loss of particularity on the part of the latter, its freedom from dependence upon any mental picture to make it clear and intelligible, its tendency to progressive differentiation, and its highest development in the form of a constructive principle, or true scientific concept. No constructive function is ascribed to the logical concept in its lower, more nearly empirical form. We are told that this higher order of concepts in virtue of having a constructive function is radically distinct from the other orders. But is not this failure of the logical concept to come into its own until so late in its career due not to its earlier proximity to empirical concept, but to its artificial isolation from it?

Berkeley's contention, that the loss of particularity on the part of the concept tends to reduce it to a dead and empty form, is answered by Professor Hibben in a way that recalls Mill's famous definition of matter. "The particular attribute of color, or of form, or of habit is indeed dropped out of mind in framing the concept, but there is always a compensation for the loss of the particular by substituting in its place the possibility, not only of the attribute in question, but all others of the same general kind. Instead of the particular we have the potential which admits of an indefinite degree of variation. Thus the concept of a rose admits of any shade of color whatsoever which is compatible with

the whole range of experience regarding roses" (p. 17). In other words, the concept is, among other things, a sort of permanent possibility of images, as matter—according to Mill's definition—is a permanent possibility of sensations.

The various types of judgments—universal, singular and negative—and the varieties of judgment forms—categorical, hypothetical and disjunctive—receive ample description and illustration. It is worth noting that the latter are treated genetically as various stages in the progress of knowledge. The discussion of division and classification is enriched by the presentation for comparison of Bacon's classification of human knowledge and Comte's and Spencer's classifications of the sciences.

This brings us to an important chapter on the nature of inference. Professor Hibben undertakes to maintain throughout his treatment of inference a sharp distinction between the psychological and the logical, between inference as a psychical experience, as something going on in some one's mind, open to observation and description, and inference as having validity, warrant, ground. The chapter opens with a very brief but admirably clear statement of the psychology of inference as rooted in the perceptual processes. Soon the question of the objective validity of these processes arises. The shade of Hume is invoked. Refuge is finally sought in 'the fundamental postulate of knowledge which we are constrained to assume, namely, that our consciousness must be self-consistent throughout' (p. 90), and in the assumption that the data of consciousness contain *within themselves* that which enables the mind to transcend them. In two or three places in this chapter the distinction between the psychological and the logical shows signs of becoming a division of labor rather than a barrier, but one dare not urge this point very far, lest it turn out to be a case of 'subjective necessity' rather than a case of 'objective validity.'

Most of the remainder of the deductive part is taken up with explanation of the more typical aspects of formal logic, the 'laws' of identity, contradiction, excluded middle and sufficient reason, the various transformations of the judgment forms, the syllogism with its mood and figure. There is a chapter on extra-syllogistic reasoning, and one on fallacies. It is an evidence of a very high order of ingenuity that the author should have succeeded in squeezing out of the various combinations and permutations of *A, E, I, O* two practical suggestions, one of them aimed against hasty generalization, and the other a useful rule in debate—'never attempt to prove more than is necessary to overthrow your opponent's main contention' (p. 109).

Part II., the 'Inductive Logic,' is introduced by a discussion of the relations between induction and deduction, in which it is made clear that Professor Hibben desires the student to understand thoroughly that the two are mutually dependent. The premises of deduction are not given ready-made, but must be constructed by us through our interpretations of reality. Mr. Bradley's argument that there can be no such thing as induction, because it always rests on an implied universal which gives to

the process as a whole a deductive character, is taken by Professor Hibben to have the force 'only of proving that induction can not be independent of deduction' (p. 173). A chapter is devoted to each one of Mill's five inductive methods. Under predication and verification the author treats of the tendency of laws or generalizations reached through inductive processes to become anticipatory. Thus science escapes the bounds of Baconian induction. It is largely through these anticipations that the progress of science is attained. The chapter following is naturally, one might say logically, devoted to the hypothesis. The double function of the hypothesis is clearly brought out—how it functions preliminary to experiment as a selecting agency, and how it functions subsequent to a problematic occurrence as an explanatory, unifying agency. It is noted how large a part is played by the imagination in the development of a hypothesis, Tyndall's thesis to this effect in his discussion of the 'Scientific Use of the Imagination' being well illustrated by quoting an instance of one of his own scientific explanations. Following the chapter on the hypothesis is a chapter on analogy, in which the important rôle analogy has played as an instrument of discovery and of classification in scientific research is made clearly evident, and five requirements of true analogy duly set forth. Corresponding to the discussion of extra-syllogistic reasoning in Part I. is a discussion, under the head of probability, of cases that are so complex that they can not be subjected to the ordinary inductive methods. Inductive fallacies are comprehensively discussed under the four rubrics of errors of perception, errors of judgment, errors of the imagination and errors of the conceptual processes. They are characterized as 'due to the psychological disturbance of our normal logical processes.' We are told that the chapter is devoted to the pointing out of errors that are mainly psychological in their origin, for two reasons. First, because the data of inference are psychological as regards their source; if observation and sense-perception be contaminated, all the subsequent processes of hypothesis, classification, etc., will suffer accordingly. Second, fallacies that are formal are not so apt to mislead. 'In the material data especially lurk the germs of fallacy.'

The book closes with a chapter on the inductive methods as applied to the various sciences, and with a historical sketch of induction, which goes back to Socrates, Plato and Aristotle, and which contains brief accounts of the logical significance of Roger Bacon, Da Vinci, Telesius, Campanella, Locke, Newton, Herschel and Whewell, as well as of Francis Bacon and John Stuart Mill. A list of nearly two hundred brief logical exercises is appended. Special mention should also be made of the abundant and interesting illustrative material in Part II., much of it drawn from the experimental sciences and adding not a little to the teaching value of the book.

The function of the book is clearly to serve as a comprehensive textbook or manual, a museum of the past and a field survey of the present. So completely has this function been fulfilled, so close and faithful is its exposition, especially of the present position of what is commonly re-

garded as logical theory, that the book has more than a pedagogical significance. It has succeeded so well that it can hardly fail to challenge criticism, the criticism of that which it has exposed.

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The Color Sensitivity of the Peripheral Retina. JOHN WALLACE BAIRD. Washington: Carnegie Institution. 1905. Pp. 80.

This paper commences with a summary of the experimental work done on the sensitiveness of the retina to color, which may be called as good as perfect. It is concise and gives the gist of each of the experiments in their chronological order and with careful references to the original places of publication. Two general conclusions can be drawn from this mass of work: "All colors tend to assume a yellowish or a bluish tint as they approach the periphery; yellow and blue do not themselves change in tone, but the less refrangible end of the spectrum—the reds, the oranges and the greens—together with the more reddish purples, appear more and more yellowish as they recede from the visual axis; while the more refrangible end of the spectrum—the violets—together with the violetish purples, pass over into bluish or even blue, at but a slight distance from the fovea. . . . The saturations of all color stimuli appear to diminish gradually and steadily, from the fovea to the outer limits of vision; in addition to the yellow and the blue, a certain tone of red and a certain tone of green undergo no change of color in indirect vision." The three experimental papers, by Bull, Hess and Hegg, which have observed all the necessary precautions, show, furthermore, that 'the retinal sensitivity to a certain red is coextensive with that to a certain green; the sensitivity to a certain yellow is coextensive with that to a certain blue; the former pair of color zones is much narrower than the latter pair.'

It seems also well established that all the color zones increase with an increase in the saturation, area and intensity of the stimulus. In fact, "the more recent workers at least, are of the opinion that even the peripheral retina is not, strictly speaking, color-blind. Its 'color sense' may be so weak that a relatively strong stimulation is required to call forth the whole manifold of sensation qualities which it is capable of furnishing; but that it may, under appropriate conditions, furnish the same quality series as the more acute central regions is no longer doubted." This should seem to bear interestingly on von Kries's theory of peripheral vision, and Baird does not hesitate to say that 'it seems evident throughout that the behavior of the eccentric regions of the retina has given von Kries an endless amount of trouble; and his own discussion shows that his theory is wholly inadequate to account for the phenomena in question.'

The experimental portion of Baird's paper deals with two questions, the chromatic character of the sensation aroused when a constant color stimulus is applied successively to different regions of the retina, and the relative extension of the retinal areas within which the tones of the different color stimuli are correctly recognized. The list of the various

changes in sensation aroused by the various color stimuli in passing from the fovea to the periphery is too long to give here, but Baird concludes that 'if the stimulation be sufficiently intensive, all colors may be recognized at the extreme periphery.' It was incidentally found that a peripheral stimulus applied constantly and continuously gives a sensation which is by no means constant, but decreases in saturation and frequently changes in color tone as well. These changes are tabulated. Previous stimulations of which the after-images have entirely lapsed may, nevertheless, modify the color tones of succeeding stimulations.

Baird rightly insists that any determination of the relative extension of different retinal zones is worthless unless such colors are used as do not change in tone while they move from the fovea to the periphery (*i. e.*, are physiologically stable); and especially such as are equal in intensity and in saturation. "The equating of the white-values was accomplished by comparing the brightness of the gray sensation aroused by each upon the peripheral retina." The quality of color values was insured by the particularly clever device of mixing these colors, already white-equated, on a color disk and changing the color values until the mixture was a perfect gray. If then a white-value was found to have been altered it was corrected, and the process repeated until by successive approximations four colors were obtained (red, yellow, green and blue) which were equal in both intensity and saturation. "Eight half-meridians were explored," and "the results show that the zone of stable red is coextensive with that of stable green, that the zone of stable yellow is coextensive with that of stable blue, that the yellow-blue zone is much more widely extended in all directions than is the red-green zone."

The experimental work seems to have been done with admirable care and the paper is written up with that conciseness and excellence of style that have characterized such of the publications of the Carnegie Institution as have come under this reviewer's eye.

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Psicologia Fisiologica. G. Mantovani. Milan: Ulrico Hoepli. 1905. Pp. 175.

We have here what is seldom attempted, a real primer of physiological psychology. Naturally the book can have value for few outside of the Italian public, but for Italians who want to find out what sort of a study modern psychology is, this tiny manual should be excellent. It is extremely elementary, of course, but surprisingly solid for a book of its size and purpose. The author in his preface to the first edition (1896) regrets that psychology is little cultivated in Italy, and that the public is either unaware that such a study exists or has a wholly mistaken idea of it. Dr. Mantovani hopes only to spread a little the true notion of the experimental study of mental facts, and to incite some to the reading of larger books. As psychology in Italy is to-day far from negligible, and as a second edition of this little book has been called for after nine years, it may have had its measure of success.

Any foreigner who for the sake of his Italian wants a psychological reading-book may take this. The style is clear, and the doctrine, what there is of it, sound.

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Identität und Gleichheit mit Beiträgen zur Lehre von den Mannigfaltigkeiten. KURT GEISSLER. *Zeitschrift für Philosophie und Philosophische Kritik,* Band 126, Heft 2, pp. 168–188.

The question of the significance of the law of identity $A = A$ is here treated with reference to the writer's previous investigations in the field of mathematics. Although the latter are, strictly speaking, outside the realm of pure logic, they may, nevertheless, prove of assistance in determining the vexed question of identity. The identical equation in mathematics $A = A$ is the equivalent of $A - A = 0$, and accordingly the problem of the meaning of identity is closely connected with that of the meaning of zero. According to the theory of the *Weitenbehaftungen* both zero and the mathematical point are relative terms. For instance, the tangent of a circle has an infinitely small portion in common with the circumference. For the finite or sensible *Weitenbehaftung* this distance may be neglected, it is a point, but for the lower *Weitenbehaftung,* that below sense perception, it is a definite distance, comparable with other distances and no more to be neglected in an estimate of spatial relations than is the circle's circumference in the field of the finite. The same thing is true of the higher *Weitenbehaftung,* that of the infinitely great. Here finite distances become points of no spatial value. The doctrine of relativity holds for numbers as well as for space. Zero is the expression of a quantity which is by no means the equivalent of nothing, but which, belonging to a lower *Behaftung,* is incomparable with the others with which it is used, and so may be ignored in equations.

The conclusion, though not definitely stated, is evident. If the mathematical point and zero are both relative conceptions, denoting quantities which in the case in question may and must be ignored but which are no less existent than the sides of a triangle or the number ten, then the law of identity, which is capable of mathematical expression, must be also relative. Possibly logic, like mathematics, deals with different *Behaftungen.* The suggestion is an interesting one and it is to be regretted that the author has employed such brevity with regard to the philosophical applications of his mathematical doctrines.

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WELLS COLLEGE.

JOURNALS AND NEW BOOKS

ZEITSCHRIFT FÜR PSYCHOLOGIE UND PHYSIOLOGIE DER SINNESORGANE, July, 1905, Band 39, Heft 1 u. 2. *Experimentelle Beiträge zur Psychologie des Schlafes* (pp. 1–41): WILHELM WEYGANDT. – A determination of the capacity for mental work, as meas-

ured by addition and memorizing, after various periods of sleep. For the performance of practiced work, such as addition, a half hour's sleep gives full restoration. For more difficult work, such as memorizing, a longer period of sleep is necessary. The restorative effect of sleep is, in the case of difficult mental work, proportional to the duration of it. *Das Augenmass bei Schulkindern* (pp. 42–87): HERMAN GIERING. – The chief result of this study is the absence of improvement in eye-measurement in children of school age and even in many cases from the age of three. Boys appear to be superior to girls. In general children from six years and upwards are subject to the same illusions as adults. When experiential data are excluded differences of depth are not distinguished in monocular vision. *Ueber die Bleichung des Sehpurpurs durch Lichter verschiedener Wellenlänge* (pp. 88–92): W. NAGEL und H. PIPER. – Experiments on the retinæ of owls and frogs showed that the series of colors produced in the bleaching of the visual purple is unaffected by the wavelength of the bleaching light. No support found for Kühne's assumption of a visual yellow. *Dichromatische Fovea, trichromatische Peripherie* (pp. 93–101): WILIBALD NAGEL. – Description of a case of green-blindness of the fovea with trichromatic peripheral vision. It goes to show the unsatisfactoriness of the Holmgren test which proved ineffective in disclosing the defect. *Ueber die Verlegung der Netzhautbilder nach aussen* (pp. 102–110): A. E. FICK. – Outward projection of a single luminous point in a dark visual field is extremely inexact, contrary to usual assumptions. Literaturbericht.

Adler, Felix. *The Essentials of Spirituality.* New York: James Pott & Co. 1905. Pp. 92. $1.00.

Hegel. *Encyclopädie der philosophischen Wissenschaften im Grundriss.* Edited by Georg Lasson. Leipzig: Darr. 1905. Pp. lxxvi + 522. 3.60 M.

Lagerborg, Rolf. *Das Gefühlsproblem.* Leipzig: Barth. 1905. Pp. vi + 141.

Lukas, F. *Psychologie der niedersten Tiere.* Vienna and Leipzig: Braummüller. 1905. Pp. 276.

NOTES AND NEWS

THE experiments of Professor Burke in artificial biogenesis (see the *Fortnightly Review* for September) emphasize the revolutionary nature of much of the progress in present day biology. Commenting on these experiments, the *Medical News* notes investigations and results of other workers in the same field. Bütschli was able to obtain combinations which showed activities much like those of protoplasm. Artificial emulsions showed a microscopic structure almost identical with that of protoplasm; oil emulsions suspended in water showed ameboid changes of form. Quincke's experiment is classic. He suspended in water a drop of almond

oil and chloroform, and then by means of a capillary tube caused a drop of a two per cent. solution of sodium carbonate to approach the globule. The latter exhibited movements strikingly like those of amœbæ. The latest of these attempts has been made by Stephane Leduc who produced an artificial cell showing growth and germination. In a solution of copper sulphate he suspended a drop of saccharose solution containing a trace of ferrocyanide of potassium. The drop was soon surrounded by a thin membrane of ferrocyanide of copper. This membrane was permeable to water, but not to sugar. The result was that the drop imbided water and increased in size. It was also seen to put out prolongations similar to the pseudopodia of amœbæ. As soon as the surface tension of the drop exceeded the internal cohesion, self division occurred, and each resulting half surrounded by a new pellicle of ferrocyanide of copper began to increase in size and then subdivide like its parent drop. R. Dubois, of Lyons, has recently observed bodies similar to the radiobes, to which he has given the name 'vacuolides.' Professor Loeb has shown that fertilization can be artificially brought about and the actions of bodily ferments have been closely imitated by the various metallic enzymes. The *Medical News* concludes its comment as follows: "About ten years ago the world was startled by the announcement from the chemical laboratory of Professor Moissan, of Paris, that the latter had succeeded by means of high pressure and the intense heat of the electric furnace in manufacturing diamonds from amorphous carbon. Although too small to be of great commercial value, the diamonds thus produced were no different from those dug out of the earth. Moissan had merely imitated nature's own process. With the same agent at his disposal, namely great heat and pressure, he succeeded in producing on a small scale the remarkable metamorphosis of carbon which nature, on a larger scale, had accomplished in her vast terrestial laboratory. With a slight stretch of the imagination one may compare the researches of Burke with those of Moissan. Employing a source of energy which is known to have existed at a time when the earth was gradually cooling from its incandescent state, namely radioactivity, and which may have participated in the transformation of the lifeless clod into the pulsating organism, he has imitated nature's handiwork. The radiobes of Burke may thus be regarded as bearing the same relation to the non-living compounds of the sterilized bouillon that the diamonds of Moissan bear to the pieces of structureless charcoal."

PROFESSOR WILLIAM OSTWALD, of Leipzig, who is to give courses in physical chemistry and philosophy, at Harvard University, during the first half of the present academic year is now in Cambridge.